General answer

We thank both reviewers for their constructive and detailed feedback. In response, we have substantially revised the manuscript to better align its scope, structure, and interpretation with the expectations set by the title and abstract. The title and abstract were revised to reflect more accurately the balance between atmospheric, landscape, and anthropogenic controls on fire size. The Results section was strengthened, particularly the Random Forest and SHAP analyses, including clearer interpretation of partial dependence patterns and a correction of several land-cover predictor labels (most notably distinguishing tree cover from the tree–shrub–herbaceous mosaic class). This correction did not alter the overall results or conclusions.

In addition, the Discussion was completely restructured and substantially expanded to integrate vegetation structure, landscape configuration, and human fire use more explicitly, and to clarify the indirect pathways through which anthropogenic pressures influence fire size. The Methods section was revised to improve clarity, transparency, and consistency in terminology and assumptions. As a result, the manuscript is longer than the original version, but it is now more clearly structured, better contextualized within the Gran Chaco literature, and scientifically more robust.

<u>REVIEWER 1</u>

What Controls Fire Size in the South American Gran Chaco? Exploring Atmospheric, Landscape, and Anthropogenic Drivers

The research article tackles an important question regarding the controls of fire size (burned area) in the two different parts (xeric and mesic) of the Gran Chaco. It is an important subject, focusing on a special and diverse region and ecosystem. In methodology, the researchers use different approaches throughout their analyses, and the article is well written. However, the connection between fires in the Gran Chaco, their impact, and the underlying human–ecosystem dynamics is poorly established. I recommend acceptance of the manuscript following the revisions below.

Major revision.

Response:

We agree and addressed this directly by expanding the regional context and strengthening the human–ecosystem narrative. The revised Discussion now contains a substantially expanded Section 4.5 on human fire use, land clearing, and management contexts, clarifying that humans drive most ignitions, while landscape structure and fire-conducive weather largely determine final fire size. We also added clearer statements about the scope and limits of remote-sensing approaches for disentangling intentional burns, escaped burns, and wildfires at event scale.

General Comments

Since fire, especially in complex ecosystems such as the Chaco, is a product of vegetation composition, and since the authors specified landscape as one of their focus areas, I expected to see more on the vegetation types in the region — not just a general grouping of herbaceous, shrubland, etc., but specific taxa with a fire history and/or adaptation, and how they differ between the different parts of the Gran Chaco — as well as the interplay between vegetation, topography, and fire.

Response:

We expanded the Introduction and Discussion to provide additional vegetation and fuel-structure context, emphasizing how fuel continuity, productivity gradients, and land-cover mosaics condition fire spread. Because our analysis is based on consistent multi-year remote-sensing predictors across three countries, we cannot provide species-level fire adaptations at regional scale with the same rigor as local field studies. Instead, we strengthened the mechanistic bridge between land-cover mosaics, fuel continuity, and topographic gradients, and we cite Chaco-focused literature to support these interpretations.

However, the article centers its focus on meteorological conditions, leaving the human and landscape elements aside. This is quite unfortunate and represents a lost opportunity to study fire behavior in this truly magnificent landscape from different perspectives. At this stage, the title falls short of meeting expectations.

Response:

We addressed this in two ways. First, we revised the manuscript structure to ensure that landscape structure and human-pressure results are not treated as secondary, but explicitly interpreted in the Results and Discussion. Second, we revised the title and abstract to ensure the framing matches the actual balance of atmospheric, landscape, and anthropogenic evidence. The revised abstract emphasizes that the Random Forest analysis consistently ranks landscape structure as the dominant predictor set for final fire size outcomes, ahead of meteorological and demographic proxies.

Given the special and diverse nature of the area, a short history of past human–landscape interactions and how they resonate in its current architecture would be informative for readers unfamiliar with the region. This is especially relevant if human use of fire and anthropogenic ignitions have changed over time. Including this would fulfill the expectations raised by the title.

Response:

We modified the discussion (section 4.5) to include a clearer synthesis of historical and contemporary fire use (pastoral management, land clearing, residue burning, and informal fire practices). We also clarified that while the remote-sensing record captures fire footprints and correlates, it does not directly encode the institutional and cultural evolution of ignition practices. We therefore cite regional syntheses and ethnographic sources, and we explicitly motivate future work integrating social data and ignition records.

The authors have meticulously resolved the meteorological parameters, but the use of FireCCI51 is unfortunate. I strongly urge the authors to compare results with the updated version, FireCCI511. (See line-specific comments below.) Additionally, the statistical methods used should take into account their underlying assumptions and be reconsidered where criteria are not met.

Response:

We clarified FireCCI provenance and version equivalence. FRY v2.0 was produced from FireCCI51 distributed via ESA CCI, and the corrected content is equivalent to the Copernicus CDS FireCCI v5.1.1cds stream. This equivalence and the reason for differing version labels has been confirmed in writing by M. Lucrecia Pettinari, who explained that ESA CCI replaced the affected files in place under the same FireCCI51 label, while CDS issued a new version identifier (v5.1.1cds) for traceability.

We also strengthened the methodological justification for correlation analyses by clarifying that we correlate monthly anomalies rather than raw burned area, and we tightened the inclusion criteria for pixels (minimum number of fire-active months). In addition, following the reviewers' concerns, we added a robustness check using Spearman rank correlations and report that the main spatial patterns remain consistent

The scale considered for megafires should also be revisited and explained — for example, why ~10,000 ha is selected instead of a larger number (e.g., 40,000+ ha), which many agencies define as the minimum for a megafire. An overall assessment of the impact of these fires is also missing and would be beneficial.

Response:

We revised the text to make the rationale explicit. Our megafire and gigafire definitions follow Linley et al. (2022), proposed specifically to harmonize terminology across studies and avoid inconsistent agency thresholds. We added a clearer statement that agency thresholds vary, and we justify our choices as a standardized scientific classification for global comparability rather than an operational fire-management definition.

We already know from previous research that fire severity and burned area are influenced by meteorological conditions, climate variability (e.g., anomalies in wet/dry periods), fuel build-up, wildland–urban interface, etc. However, while the manuscript presents lengthy analyses of meteorological conditions, the human influence is weakly suggested or entirely missing.

Response:

We addressed this by expanding and restructuring Section 4.5. The revised Discussion explicitly explains why human-pressure layers may show limited incremental explanatory power for final fire size once landscape structure is included: human influence is largely embedded in long-term land-use change, fragmentation, and fuel restructuring, and coarse proxies cannot distinguish intentional versus escaped burns. We also improved interpretation of SHAP gradients for cattle density, road density, and population density, linking patterns to accessibility, fragmentation, detection, and suppression potential.

The researchers may wish to change the title and modify the abstract. Leaving out the anthropogenic element — in a region with such pronounced and changing human impact — would negatively affect the novelty of the research. Considering that the largest driver of deforestation in the region is anthropogenic, and that there is a clear human impact on fires in the Chaco, a better-established discussion and analysis of human drivers is expected.

Response:

As mentioned before, we revised the title and abstract to ensure the framing matches the actual balance of atmospheric, landscape, and anthropogenic evidence.

In section 4.5, a weak reference to the human element is made, but it falls short of a literature review. The only reference to indigenous fire practices (or agriculture) appears in the introduction and is not developed further, which is unfortunate. Similarly, the landscape–vegetation connection is brief and weak. If these aspects remain underdeveloped, *it might be better to change the title and focus more on climate variability and anomalous fuel build-up.*

Response:

Section 4.5 was rewritten into a structured narrative: (i) fire use and ignition contexts, (ii) land clearing and agricultural expansion, (iii) informal and semi-prescribed burns and monitoring gaps, (iv) indirect pathways by which human pressures shape fire size through fragmentation and fuel structure. We also introduced an explicit clarification about exotic grasses to avoid overgeneralization and to align with the reviewer's expectation that claims be grounded and region-appropriate.

Finally, the presentation of results needs more structure. If the authors wish to pursue a meteorology–vegetation–human narrative, they should include more detail on the latter two drivers. Currently, the manuscript reads as a detailed analysis of meteorological conditions with only a generalized treatment of the other two potential drivers.

The authors train Random Forest (RF) models, and the results indicate that land cover and vegetation rank higher in importance than wind and precipitation. This should be emphasized.

In section 4.6, the authors list these omissions as limitations, but if so, the study design and title should have been changed. Moreover, the authors have not fully used the datasets available to them to analyze the interaction between wildfire and human presence.

Response:

We improved internal structure by tightening subsection transitions, reducing repetition, and emphasizing the link between Results and Discussion mechanisms. We also explicitly highlight that Random Forest results rank vegetation and topography higher than wind and precipitation, and we carried that emphasis consistently into the abstract and Discussion. We also extended the limitations and perspectives analysis.

Line-Specific Notes / Corrections

Line 70: "indigenous" should not be capitalized.

> **Response:**
> Changed

Line 73: Clarify "landscape context to assess how fire size responds to both short-term anomalies and long-term…" — meteorological extremes? Climate anomalies? Specify.

> **Response:**
>
> Revised to specify the intended meaning as long-term environmental gradients (fuel structure, land-cover mosaics, topography, and accessibility proxies) versus short-term meteorological anomalies (FWI-related variability and wind conditions).

Line 76: FireCCI51 is outdated. Use the updated FireCCI511cds from Copernicus Data Store (CDS) or compare both versions to identify potential differences affecting results.

During July 2020, errors were found in some v5.1cds files. These were fixed, and a new version (v5.1.1cds) was created to replace v5.1cds. Only version v5.1.1cds should be used.

> **Response:**
>
> We added an explicit provenance note stating that ESA CCI FireCCI51 content is equivalent to CDS v5.1.1cds, and that FRY v2.0 was computed from the corrected CCI dataset. We cite the written confirmation from the FireCCI team.

Line 193: Clarify your use of "mega" and "giga"-fires. Define the scale used (e.g., 100+ km^2 ≈ 10,000 ha is large, but not a "megafire" if 40,000 ha is the accepted minimum).

Line 226: Define "very" small/large. Alternatively, use "smaller" or "larger" events and provide spatial context (1–100 km^2).

> **Response:**
>
> Addressed above.

Figure 3: The x-axis labels are barely readable. Also, explain the clear peak in 2020 and the downward ignition trend from 2003–2019 followed by this spike — was it due to policy changes?

Response:

We improved readability and moved the duration subplot to supplementary.

Line 373: The peak in 2021 is attributed to fire-weather anomalies, but this is insufficient. Considering that >80% of wildfires globally originate from human ignitions, linking an anomalous year solely to weather omits a major part of the story.

Lines 398–399: You use Pearson correlation, which assumes a normal distribution, but previously noted that burned area is skewed (suggesting a Poisson distribution).

Response:

We clarified that correlations are computed on anomalies and we added a Spearman robustness analysis. We also tightened pixel filtering to reduce the influence of highly sparse or strongly skewed anomaly series.

Line 413: Clarify what you mean by FWI "anomaly." Is it the deviation from a long-term mean? Why not use FWI95 (extremes), which is often a better indicator of fire dynamics? Explain what constitutes an anomaly in your analysis.

Response:

We clarified anomalies as deviations from the 2001–2020 climatology (computed daily then aggregated to monthly), and we ensure this definition is explicit in Methods.

Line 537: RF models prioritize vegetation and topography, yet your results emphasize meteorology, which offers limited novelty. Teleconnections are barely addressed, and there *is no real analysis of climate changes in the region (e.g., is the Dry Chaco drying faster? Is the Wet Chaco experiencing more droughts?). Strengthen this discussion or adjust the focus.*

Line 620: Include a digital elevation model (DEM) to show terrain differences. Since SRTM 1 km was used, overlay it on map plots to help readers understand the terrain and burned area distribution. The discussion of vegetation's role in fire spread is too brief and generalized. Consider discussing refugia, pyro-catchments, vegetation–topography–soil interactions, and changing burn patterns.

Response:

We clarified that a DEM layer is already included where terrain context is needed (and we improved the text guidance to help readers identify it). We did not overlay DEM on all maps to avoid obscuring the main layers, but we strengthened the text interpretation of vegetation–topography interactions and how topography proxies drainage, accessibility, and fuel types. We did add 4 supplementary figures with DEM comparison to provide terrain context and support interpretation, showing how fires, land cover and topography match in the different subregions.

REVIEWER 2

What controls fire size in the South American Gran Chaco? Exploring atmospheric, landscape, and anthropogenic drivers.

General comments

The article analyzes the main drivers of fire size in the Gran Chaco forest of South America—one of the world's largest and most threatened dry forest ecosystems. It uses a fire database derived from medium-resolution remote sensing data, covering a substantial temporal span (2001–2022) suitable for this type of analysis. The study focuses on short- and mid-term weather conditions that may influence burned area extent and fire characteristics at a regional scale, yielding novel and insightful findings. In addition, it examines various environmental factors contributing to fire size.

I believe the manuscript would benefit from a more in-depth analysis of the results, along with a reconsideration—or at least a clearer explanation—of their interpretation. Additionally, the discussion would be strengthened by a more thorough contextualization of the findings in light of the existing literature on the ecology and environmental processes in the Gran Chaco over recent decades. While some relevant studies are cited, there are other works—albeit not covering the entire Chaco ecoregion—that could provide valuable points of comparison or support for the results presented.

> **Response:**
>
> We fully agree and addressed this by rewriting the Discussion into a more mechanistic structure and integrating additional Gran Chaco studies recommended by the reviewer. The revised Discussion explicitly contrasts subregional fire regimes, fuel constraints, and weather sensitivity, and it connects our statistical findings to documented Chaco processes such as grazing-fuel feedbacks, land clearing, and drought episodes affecting the La Plata basin.

One of my main concerns relates to the Random Forest (RF) analysis. I am not fully convinced that elevation is truly the most important variable explaining fire size. Although *the manuscript acknowledges that elevation is not a direct driver, the current explanation of its role lacks depth. Given that elevation consistently appears as a key predictor across all subregions and seasons, the authors should explore more thoroughly the ecological and land-use processes that might underlie this result. At present, the argument that elevation reflects broad ecological gradients in vegetation composition, fuel moisture regimes, and land-use history is too vague to be convincing. I strongly suggest repeating the analysis excluding elevation as an independent variable to better understand the role of other, potentially more direct, drivers.*

Response:

We repeated the Random Forest analysis excluding elevation (and slope) to test whether the dominance of topography masks other drivers. The revised Results report that removing elevation does not substantially alter the ranking of the remaining predictors, indicating robustness. We also expanded the Discussion substantially to provide an evidence-based interpretation of why elevation is a high-importance predictor across subregions: it acts as an integrative proxy capturing drainage and floodplain structure in the Wet Chaco, accessibility and settlement patterns in some uplands, and vegetation type plus suppression difficulty in the mountainous Very Dry Chaco.

That said, I believe the study has strong potential, particularly given the wide range of variables included and the novelty of this type of analysis for the Gran Chaco. I encourage the authors to strengthen the connections between their findings and the existing local literature relevant to the study area.

The article requires Major revisions to be accepted.

Specific comments

L42-44: I believe this statement is not sufficiently supported by the references cited. If there is evidence to back this claim, the authors should explain the connection more clearly. Specifically, are there studies demonstrating that areas invaded by exotic grasses tend to experience larger or more intense fires? Clarifying this link is essential to strengthen the argument and ensure it is grounded in existing research.

The article by D'Antonio, as well as Bravo et al. (2014), may indeed support a potential relationship between exotic grasses and increased fire intensity. However, to substantiate this argument in the context of the Gran Chaco, it is important to clarify how extensive these exotic grass-invaded areas actually are within the region. Such areas would need to be considerable in extent to justify the strength of the statement currently made in the manuscript. Moreover, there may be other underlying ecological or land-use processes—currently not discussed—that could more convincingly explain observed changes in fire regimes. Including these considerations would significantly strengthen the interpretation of your results.

Response:

We revised the relevant sentences and sources to avoid overgeneralization. We now state that exotic grasses can enhance fuel continuity and fire intensity in dryland systems, but we explicitly acknowledge that their spatial extent and dominance in the Gran Chaco are heterogeneous and poorly constrained at regional scale. We therefore present exotic grasses as one plausible mechanism among several, rather than a dominant driver of large fires.

The article by Naval Fernández et al. 2023 may support the idea of climate variability and larger fires: Naval Fernández, M. C., Albornoz, J. V., Bellis, L. M., Baldini, C., Arcamone, J. R., Silvetti, L. E., ... & Argañaraz, J. P. (2023). Megaincendios 2020 en Córdoba: Incidencia del fuego en áreas de valor ecológico y socioeconómico. Ecología Austral 33: 136-151.

L45: Here you refer to traditional land management or suppression strategies? I suggest rewritting these sentence to make your idea clearer.

L51-52: I suggest rewritting this sentence as Kelley et al. and Jones et al do not specifically focus in the Gran Chaco.

L68-69: Please, specify the characteristics of the environment in which the study of Saucedo and Kurtz was performed, as it is a particular environment of the Gran Chaco.

L92: climate or weather?

L97, Section 2.1. The introduction or Section 2.1 should describe better available knowledge about the fire regimes in the Gran Chaco ecoregion.

Also, it is not recommended to begin a section with a figure or table. This comment applies to the whole manuscript.

L114: clarify that these are Argentinean provinces, as readers from other countries may not be familiar with them.

Response:

All suggestions were addressed.

L155-158: Please indicate at least the spatial resolution of the land cover product. Also, please explain why the ESA product was a better choice than annual land use-land cover maps from MapBiomas Chaco? https://chaco.mapbiomas.org/ . Global products may have significant errors in local to regional studies.

Response:

We expanded Methods to explicitly state the land-cover resolution and to justify ESA CCI MRLC as a harmonized, temporally consistent product aligned with FireCCI/FRY workflows for a multi-country, multi-year analysis. We also acknowledge that regional products may offer advantages locally, and we note that future work could evaluate sensitivity to alternative land-cover sources where consistent time series are available.

L192: Two terms are used in the same way: Fire Polygons and Fire Patches (FPs). I recommend using the same term along the manuscript.

Response:

Fixed.

L267: change “shaping fire activity” by “determining fire size”

L271: Here you mention fire frequency, but the Section's title only refers to fire size.

Response:

This was clarified.

L275, Table 1: Please, provide more detail in the Table's Caption, to be more interpretable outside of the context of the manuscript. For instance, you might mention that these are potential predictor variables of fire size in the Gran Chaco.

Table 1: Instead of "Climatic" it should be "meteorological"

Table 1: Did you obtained Land cover composition variables from the map of same year as the fire patch? I guess you did, but it would be better to explicitly mention this, as this is a dynamic variable. The same would apply for Landscape heterogeneity variables.

Response:

Yes. And this was clarified in the text.

L289: You refer to "predictor variables" and so far, they are independent or potential predictors until the statistical analysis confirms their predictive value.

L309, Figure 3: "ignitions" in panel b, refer to the fire counts mentioned in section 2.3.3? this is confusing. Then, in lines 312 and 2013 you mention "number of fire polygons". I recommend using fire counts, as ignitions are probably much higher.

Response:

The above suggestions were addressed.

L314-316: Linear regressions were not mentioned in the Materials and Method section. The whole sentence is quite confusing, since fire extent may refer to individual fires and here it is associated with ignition frequency.

L317: "suggesting a greater role of other drivers in the later". I don't think you should use the term "drivers" here, as you are not studying the drivers of annual burned area. The weaker association would be caused by despair fire sizes through time, which is confirmed in Figure 5.

L318: Isn't it R2?

L320: I find quite confusing the use of "ignition frequency" to refer to fire count. I recommend changing the expression.

Response:

Fixed.

L350: According to Figure A3, Wet Chaco had 2 Gigafires (GF) and Very Dry Chaco had 1GF. However, you include very dry chaco in the same level as the Dry Chaco.

L367: I would recommend not using the term fire frequency to refer to the number of fires, as it may be confusing with the fire metric that refers to the number of times an area burned in a certain period of time.

L393: isn't it September? (instead of October)

Figure 10: I suggest using some level of transparency, so when they overlap a more intense color would appear.

L492-497: Further interpretation of results is needed here…what does this means? the text focused solely on percentages, which are available at Fig. 10, but I would like the authors explain the meaning of these results. Also, I wonder if wind driven fires are larger, as the green color in Figure 10 highlights more than the other FWT.

Figure 11: It would be helpful for readers if you use the same colors for each FWT in the different figures.

Response:

We expanded the Results interpretation for Fire Weather Types and clarified that the strongest signal for morphology is tied to during-fire wind persistence. We also ensured color consistency across FWT figures and added explicit definitions for morphology metrics in Methods, clarifying why an elongated shape can coincide with lower perimeter-to-area under certain cohesion and smoothing characteristics, depending on metric definitions.

L509-514: I agree that Figure 10 shows some interesting results of comparing pre- and during-fire clusters, but it is not clear to me to repeat this in Figure 11, as you analyze weather data ± 20 days before and after ignition. In the pre-fire cluster, the time after ignition corresponds to the time of "during fire", and the opposite applies to the during-fire cluster. Also, your analysis of Figure 11 seems to focus on the pre-fire cluster. For instance, wind speed after ignition in the during-fire cluster is striking, but nothing is mentioned about it in the text.

Response:

Figure 11 was retained, but both its visual design and interpretation were revised for clarity. Figures 10 and 11 serve complementary purposes. Figure 10 compares clusters derived exclusively from pre-fire and during-fire conditions, following approaches used in previous studies, and highlights how distinct atmospheric regimes characterize large fires. Figure 11, in contrast, uses the same fire polygons and examines the ±20-day temporal evolution of the same meteorological variables, but reorganized according to pre-fire versus during-fire clustering. This allows us to directly assess how grouping the same polygons based on different temporal windows or variables modifies the apparent signal in the surrounding time series.

L515-519: The meaning of each morphology index should be explained in the Materials and Methods section. Without the exact definition of each. It stands out to me that wind driven fires are more elongated, but have higher core-area index and lower perimeter-to-area ratio. Then, what does elongated mean? I would think that an elongated fire would have higher perimeter-to-area ratio.

Response:

This was clarified.

L524, Section 3.6: There is no metric/index providing information about the goodness of your RF model/s, and this would be valuable to interpret and discuss your results.

Response:

We added Random Forest performance metrics (OOB R^2 and RMSE) in the Results, and we clarified the training and evaluation setup to support interpretation of variable importance and SHAP patterns.

L525-527: this paragraph is not necessary as it contains information from the M&M section.

Response:

We removed this paragraph from the Results section. Redundant methodological descriptions are now restricted to the Materials and Methods, improving conciseness and readability.

Figure 13, Caption: Focus on the meaning. In this figure you want to show the contribution of each variable. Then I suggest to focus the caption with the concept and then clarify that you used SHAP.

Response:

The caption was rewritten to emphasize the conceptual meaning of the figure, namely the relative contribution of predictors to fire size across subregions and seasons. The use of SHAP values is now introduced as the methodological tool supporting this interpretation, rather than as the primary focus of the caption.

Figure 15: what about fires at higher elevations? the plot reaches 425 m asl, but there are fires at higher elevations. Did you fix the length of the x-axis due to fewer cases? Also, what can you say about the distribution of elevation values...in your study area most land is

below 500 m asl. The same question applies to slope effects: I guess you may have a small proportion of land with steep slopes.

Response:

We clarified in both the figure caption and the Results text that the x-axes in Figure 15 are truncated at the 99.5th percentile of the predictor distributions. This choice avoids visual distortion caused by a very small number of high-elevation or steep-slope cases. We also added a short description of the elevation and slope distributions in the study area, noting that most of the Gran Chaco lies below ~500 m a.s.l. and that steep slopes occupy a limited fraction of the landscape. These clarifications are now explicitly stated to guide interpretation.

L559-561: My interpretation of LC evenness is that there are large fires (bluer dots) all along the range of this predictor variable...unless I am reading it wrong. Instead, I agree with your interpretation of tree cover. Also, in relation to this variable, the range in the figure is 0 - 1, so you seem to have used proportion, instead of percentage.

Response:

We agree with this interpretation and revised the text accordingly. We now clarify that land-cover evenness is expressed as a normalized index ranging from 0 to 1, not as a percentage. The revised interpretation emphasizes that large fires occur across a wide range of evenness values, indicating a weak monotonic relationship, in contrast to the clearer gradient observed for tree cover.

L562-564: I see other relationships in Fig 15. For example, at certain cattle density (the variable is named "number of cattle" but it is expressed as a density) you have very low fire activity, which could be associated with lower fuel availability to burn. If I am interpreting the figure wrong, please provide more detail in the manuscript for other readers.

Also, for croplands/natural mosaics, I see that larger fires occur at proportions lower than 0.5, and then large fires are less common.

Response:

We expanded the interpretation of these relationships in the Discussion. For cattle density, we now explicitly discuss the possibility that high grazing pressure reduces fine fuel continuity, leading to lower fire activity despite suitable fire weather. We also clarified that the variable represents cattle density rather than absolute numbers. For cropland–natural vegetation mosaics, we added text noting that large fires are most frequent at intermediate proportions, while highly fragmented or heavily cultivated landscapes tend to limit maximum fire size.

Also, importantly, some LC variables were wrongly named in the previous version, creating an important confusion. This was revised and fixed.

L583-589: I recommend the following papers to consider in this part of the discussion:

-Bravo, S., Kunst, C., Grau, R., & Aráoz, E. (2010). Fire–rainfall relationships in Argentine Chaco savannas. Journal of Arid Environments, 74(10), 1319-1323.

-Argañaraz, J. P., Pizarro, G. G., Zak, M., Landi, M. A., & Bellis, L. M. (2015). Human and biophysical drivers of fires in Semiarid Chaco mountains of Central Argentina. Science of the Total Environment, 520, 1-12.

Response:

All suggested references were added and integrated into the Discussion. These studies are now explicitly used to contextualize our results on rainfall variability, human drivers, and fire regimes in semiarid and mountainous sectors of the Gran Chaco.

L621-628: I am honestly not convinced that elevation is the main variable explaining fire size. While the manuscript acknowledges that elevation is not a direct driver, I would encourage the authors to explore more deeply the underlying factors that could be contributing to its apparent importance in the analysis. As elevation emerges as a key predictor across all subregions and seasons, the current explanation—that it reflects broad ecological gradients in vegetation composition, fuel moisture regimes, and land-use history—feels too general and insufficiently supported. A more detailed and evidence-based discussion is needed to clarify this interpretation and to justify the prominence of elevation in the results.

Response:

We substantially expanded the Discussion to provide a more detailed interpretation of elevation as an integrative proxy rather than a direct causal driver, and included as supplementary the figures that were shared in our previous answer. We now explicitly link elevation gradients to hydrological regimes, floodplain versus upland vegetation structure, accessibility, land-use history, and suppression potential, with distinctions among Wet, Dry, and Very Dry Chaco subregions. In addition, we report results from a sensitivity analysis excluding elevation, showing that the relative importance of other predictors remains stable, supporting the robustness of the findings.

L629-631: Here the shrub cover is mentioned as linked to large fires, but this was not highlighted when analyzing Fig. 15.

Response:

This was revised and fixed.

L635-636: There are some papers addressing fuel moisture content in certain areas of the Chaco forest that it would be valuable to consider here:

-Argañaraz, J. P., Landi, M. A., Scavuzzo, C. M., & Bellis, L. M. (2018). Determining fuel moisture thresholds to assess wildfire hazard: A contribution to an operational early warning system. PloS one, 13(10), e0204889.

-Arganaraz, J. P., Landi, M. A., Bravo, S. J., Gavier-Pizarro, G. I., Scavuzzo, C. M., & Bellis, L. M. (2016). Estimation of live fuel moisture content from MODIS images for fire danger assessment in Southern Gran Chaco. IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 9(12), 5339-5349.

-Bianchi, L., Defossé, G., Dentoni, M., Kunst, C., Ledesma, R., & Bravo, S. (2014). Dynamics of fuel moisture and its relation to the ecology and management of fire in the western Chaco region (Argentina) I: basic concepts.

L650-651: It would be valuable to compare your results with other studied performed in areas partially overlapping with your study area:

-Fischer, M. A., Di Bella, C. M., & Jobbágy, E. G. (2012). Fire patterns in central semiarid Argentina. Journal of Arid Environments, 78, 161-168.

Response:

We added the suggested references and expanded the Discussion to better connect our fire–weather findings with regional studies on live and dead fuel moisture. We also incorporated the suggested study for neighboring areas.

L665-669: I strongly disagree with this comment. While fire is indeed widely used as a land management tool, in most cases it is not applied under controlled or regulated conditions. In fact, its use is often informal or even illegal, depending on local regulations. Moreover, there are additional sources of fire ignition in the region, including hunting practices, waste burning, and accidental ignitions. The current description of prescribed burning, as well as the way the reference to Hsu et al. (2025) is presented, gives the impression that their work characterizes fire use in the Gran Chaco ecoregion—which it does not. If you wish to retain the citation of Hsu et al., I recommend rewriting the sentence to clarify that their findings do not directly reflect the specific fire management context of the Gran Chaco.

Response:

We revised this paragraph to explicitly distinguish prescribed burning in formal monitoring frameworks from the largely informal, semi-prescribed, or opportunistic burning practices reported in the Gran Chaco. We also revised the sentence citing Hsu et al. to clarify that global prescribed-fire inventories do not cover the Gran Chaco context adequately, and we use it only to motivate the broader gap in consistent inventories rather than as evidence describing the region.